

# *In Response*

## Praxics and Behaviorism

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In "The case for praxics," Epstein (1984) proposed that: (1) the term "praxics" should supplant "behavior analysis" and/or "the experimental analysis of behavior" to identify the scientific study of behavior; (2) a clear distinction be drawn between praxics and behaviorism; and (3) praxics should be separated from the field of psychology. We have no principled objection to the suggestion that alternative terms for behavioral science be considered, and agree that the term "praxics" has much to recommend it. We find, however, that Epstein's discussion of the distinction between praxics and behaviorism is not simply aimed at clearly discriminating between these two aspects of our discipline. Rather, Epstein advocates the same kind of separation of praxics from behaviorism that is proposed for praxics and psychology. In our judgment, neither of these cases for divorce is supported by strong evidence or a compelling line of argument. Because of space limitations, however, we restrict most of our comments to the praxics-behaviorism issue.

Epstein presents five arguments in support of the separation of behaviorism and praxics: (1) Watson *could* have delineated the subject matter for psychology without taking his stands on the nature of consciousness, introspection, free will, and so on; (2) the laboratory study of behavior *can* proceed almost entirely independently of the "philosophical assertions" of Watson and Skinner that are customarily viewed as being characteristic of behaviorism in general; (3) no laboratory science, no matter what its origins, should be *constrained* by a philosophy;

(4) people who have had doubts about behaviorism have kept away from behavioral laboratories or have stayed there uneasily; and (5) behaviorism is unattractive to the American public, and hence the science of behavior has suffered materially because of its close identification with it.

The first two of these arguments are truisms that cannot be denied in and of themselves. Watson perhaps *could* have argued for the objective study of behavior without concurrently denying mind; and the laboratory study of behavior *can*, and sometimes *does*, proceed independently of Watsonian or Skinnerian "philosophical assertions" (e.g., "behavior is orderly and predictable"; "free will is an illusion"; "thoughts are not the causes of behavior"; "feelings do not cause behavior"; "language is also behavior and can only be understood as such"). To argue from *what could have been* and *what sometimes is* to *what should be*, however, is a line of deduction that is flawed by precarious lapses and leaps in logic.

The third argument, "that a laboratory science should be unconstrained by philosophies," suggests that behaviorism imposes damaging restrictions on our research endeavors, but Epstein does not clearly specify the nature of those presumed constraints. It appears to us that he rejects behaviorism at times simply because he views it as a "school of philosophy" (presumably, the term "philosophy" has negative connotations for him). Similarly, it is not clear that the defining features of Skinner's *radical behaviorism* are fully appreciated (curiously, "radical behaviorism" is not mentioned as such, although Skinner is often referred to). We prefer the "world view" notion of radical behaviorism as a *comprehensive psychological theory* that includes epistemolo-

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gy, philosophy of science, principles of individual behavior, and cultural analyses as interdependent substantive components (e.g., Malagodi, 1985; Michael, 1980). From this perspective, radical behaviorism is not just an "ism" as Epstein argues—"a system of interrelated assertions and beliefs primarily about mind, free will, and feelings"—and it does not, as implied by this statement, simply *assert* that "feelings are not causes of behavior," for example, out of some vague philosophical conviction of unspecified origins. The radical behaviorist *induces*, *deduces*, and *concludes theoretical propositions* about feelings on the basis of: (1) empirical evidence (e.g., Brady, Findley, & Harris, 1971; Brady & Harris, 1977), (2) psychological theoretical analysis of respondent and operant processes involved in feelings and emotions (e.g., Skinner, 1953, 1974), and (3) epistemological analysis, based upon a psychological theory of verbal behavior (Skinner, 1945, 1957), that proposes rules designed to facilitate both empirical and theoretical inquiry.

Radical behaviorism does indeed advocate constraints on the freedom of scientific speech, but we do not see how Epstein's proposal is thereby called for. As Hineline (1980, 1984) has eloquently noted, there is no mathematical formalism available for the study of behavior; hence, our calculus is comprised of our verbal practices. How we speak and think about behavior are *essential* characteristics of our science (Branch & Malagodi, 1980). Epstein's argument that it is alright to talk about our laboratory findings or their theoretical implications in mentalistic (or even metaphysical!) terms makes as much sense for the student of behavior as it does to suggest that a physicist use inaccurate equations to describe his or her findings. The constraints that radical behaviorism imposes upon our laboratory science are those that are imposed on *all* sciences.

The fourth and fifth of Epstein's arguments are anecdotal in nature, and we therefore feel free to reply in kind. As to the fourth argument, about laboratory science, during our combined thirty-three

years of experience as teachers and scientists in academic and research settings we have seen few instances of promising researchers either repelled by or withdrawing from the laboratory because of their abhorrence of "true-believing, card-carrying" behaviorists. To the contrary, far more of our failures to attract or to keep potential researchers may be attributed to what is sometimes perceived as the dullness, drudgery, and sheer difficulty of the experimental analysis of behavior. Our interactions with some sixty or so graduate students who have majored in our Experimental Analysis of Behavior program have revealed that many more were *attracted* to it by behaviorism than by praxics. We strongly suspect that a survey of members of ABA or of Division 25 of APA would yield the same general findings. We do not know why there is this disparity between our and Epstein's conclusions on this point. Perhaps it has something to do with how behaviorism is characterized and its case presented.

The final argument presented by Epstein—what is intended to be the *coup de grâce*, we gather—is that behaviorism is unattractive to the American public, and that this has engendered undue suffering on the part of the "scientific side" of our discipline. There are a lot of things that are unattractive to the American public, if the results of surveys, polls, and voting records are to be taken as evidence. Physical and biological theories of the origins of the universe and of species apparently are unattractive to that public, as are implications of those theories—for example, the "nuclear winter" model—for political-military practices. We do not see that the unpopularity of these views has seriously limited the resources allocated in support of research in physics and biology. We share Epstein's concern about the imbalance in favor of the "cognitive sciences" in the distribution of federal research support in psychology. Conclusions other than his, however, may be reached. Among these are the following: (1) that the distinction between behaviorism and praxics should be more carefully and accurately drawn than he has

done in his case for usage of the term "praxics;" (2) that differences, even when accurately portrayed, do not necessarily call for divorce—the strengthening of either of the partners can result in the strengthening of the relationship between the two; (3) federal funding of research is not a goal unto itself—it is a means to an end; (4) if that means is not currently adequate, we must examine the limitations of current research proposals as being more responsible for our failures to attract those funds than is the general cultural abhorrence of a deterministic theory of human behavior; (5) if federal sources of research support are currently minimal and promise to diminish (as suggested by predictions from current budget-deficit considerations), then the private sector, *sometimes* under greater control of real-world immediate contingencies, should be examined more fully as a source for funds, especially for applied research; (6) finally, educating both psychologist and layman about our multi-faceted discipline is not a dubious experiment that has failed and thus calls for abandonment—it is a survival-oriented undertaking that demands our continued unremitting dedication.

We would like to conclude by presenting an equation—an exercise not uncommon in contemporary behavior analysis:

$$Px - Bh = Psy$$

Where Px = Praxics; Bh = Behaviorism; and Psy = Psychology.

This equation makes it unnecessary to counter-argue Epstein's second propos-

al—the separation of praxics from psychology. We believe that a stronger case could be made for the separation of radical behaviorism and traditional psychology, for the differences between these two in their treatments of causation, exploration, and theory are more fundamental, pervasive, and irreconcilable. Whether such a separation would be to our benefit is, of course, another issue.

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